The passage from the one rule to the next is very gradual, but never tedious; there is copious explanation, but yet we think no superfluous verbiage. Every page is to the point, and marks the writer as one who has had practically to deal with the sort of young minds for whom his work is primarily if not solely intended. The book is a thoroughly rational one, though at first sight the reader may think he has stumbled by mistake upon an elaborate treatise on the subject of dominoes, for the method of teaching is based on a combination of what has been called "palpable" with written arithmetic. In actual teaching the palpable method is the one adopted. Mr. Orme's own words are, "Teachers are strongly recommended to take every available opportunity of using suitable blocks of wood to represent units, tens, hundreds, and thousands, and I should wish it to be distinctly understood that, without these blocks or some substitute for them, the subject will become too abstract to be comprehended by those for whose sole benefit this book has been compiled" (Preface, p. 4); and on p. 7 the resemblance of each diagram to a domino suggests the remark, "Learn to play at the game called dominoes; this will teach you how to tell the number of dots in a figure very quickly."

The main divisions are occupied with units, or single unpacked things; tens, or single unpacked deca-units; hundreds, or single unpacked hecto-units; and thousands, or single unpacked kilo-units. From some of the terms here employed, it might be supposed that the work treats principally of questions concerned with metres, decimetres, and other quantities, which require an acquaintance with the metric system; but this is not the case, though it is well fitted to serve as an introduction to the use of such a system. Special reference to this system is confined to two pages of "Directions," where the dimensions of a square centimetre are represented in a figure.

The following extract from the Preface will serve to show the spirit by which the writer is actuated:—"If the teacher proceeds in this way (*i.e.*, by the palpable method of using blocks) he will be amply rewarded by finding that children, not so stupid as they are often said to be, will frequently make out processes of their own for arriving at truths, having been taught to rely not on rules, but on reason; and occasionally the youngest pupil will unconsciously show his teacher how to teach. . . . . More good will arise from the introduction of a scientific method into the teaching of the ordinary subjects of education than will accrue from object lessons, or the freely-accepted dicta of men of science." In thus letting Mr. Orme speak for himself, we shall best put before our readers the object aimed at in his book—an object which he seems to us to have compassed; and as we think it is a right one, it is on this ground we venture to recommend the work to all who may be in search of a good elementary introduction to arithmetic. If they carefully follow out the advice given, and pursue the plan laid down, they will convey a sound and accurate view of the subject, and that without wearying the young student. There is good store of simple and varied exercises in this handy volume, which may readily be curtailed or enlarged, according to each individual case. The only typographical faults we have to point out occur on pp. 30, 104, and 105, and are readily corrected. We commend the book as the work of a "cunning" arithmetician. "It is pitie that commonlie more care is had, yea, and that emonges verie wise men, to finde out rather a cunnynge man for their horse, than a cunnyng man for their children."*

OUR BOOK SHELF

Contribution to the Biology and History of the Development of the Ustilagineæ. By Dr. A. Fischer von Waldheim. Translated for the Transactions of the New York State Agricultural Society for 1870. From "Pringsheim's Jahrbücher," vol. ii. part 1, 2, 1869. (Albany, New York: 1872.)

THE section of Fungi to which the Ustilagines belong has occupied considerable attention from mycologists during the past quarter of a century. Old notions of the autonomy of species have been dispersed, and at the present time all the old genera are suspected, some are condemned, and not a few amalgamated. The present condition of the classification of the Uredinous Coniomycetes is eminently transitional; so much has been written, so many observations made that await confirmation, or require further researches to render the work complete, that no one would be rash enough to predict what another twenty years may accomplish towards settling the relations of the genera in this group to each other. From the vague notions and doubts of Unger's "Exantheme" in 1833, Leveille's researches in 1839, Tulasne's first memoir in 1847, De Bary's "Brandpilze" in 1853, Tulasne's second memoir in 1854, there has been a regular advance in the accumulation of observations and the record of facts up to the publication of Fischer von Waldheim's communication in 1869. The relations of *Trichobasis* to *Puccinia*, of some species of *Lecythea* to *Melampsora*, of others to *Phragmidium*, are admitted on every hand; but whether *Trichobasis*, *Puccinia*, *Uromyces*, and *Æcidium*, shall all give way to an amalgamated genus, in which the four forms shall be recognised as four conditions of the same plant, though accepted by some, cannot yet be considered as settled beyond a doubt. The more sceptical of mycologists suspend their judgment, and await the confirmation of certain observations. Whatever the result may be, there can be but one opinion that such men as Tulasne, Leveille, De Bary, and others, deserve all commendation for the work they have accomplished. Whilst the Uredines Proper have suffered greatly in the stability of their generic distinctions, the Ustilaginous group has at present maintained its character for the autonomy of its species. Up to the present *Tilletia*, *Ustilago*, *Thecaphora*, and *Urocystis*, seem to represent comparatively stable genera. It is not impossible that this is more *seeming* than *real*, and that future workers may reveal affinities more close than as yet are suspected. The "contribution" of Fischer von Waldheim dates as far back as 1869, although now presented for the first time in an English dress, and we have to thank our Transatlantic cousins for having accomplished this fact. The New York State Agricultural Society is wise to diffuse this and all similar information amongst its members. None are more deeply interested in the development, conditions of growth, and metamorphisms (if any) of the "Smuts" than agriculturists, and to no societies should we look with more confidence for the publication of such memoirs as the present. How far they have accomplished this belongs to the past, and has become history; what they may do in the future lies within their own power. The memoir now published commences with a very good digest of the literature of the subject, after which follow the personal observations of the author on the mycelium and spore

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* Ascham, "The Schoolmaster," Book I.

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growth in *Tilletia*, *Ustilago*, and *Urocystis*. The synopsis of the Ustilagineæ in relation to their supporting plants and the place of their spore formation will be very useful to students, as will also the counter-synopsis of the supporting plants, and the Ustilagineæ occurring on them. The details of the germination of spores, direction and character of the promycelia, the effects of moisture, light, &c., measurements of threads and spores, all combine to render this a useful contribution to the literature of the Smuts, although not containing any startling discoveries. It is just what it professes to be, the record of observations on the germination of the spores of several of the Ustilagines under artificial cultivation, as a supplement to Tulasne's memoir in which this history of development was deficient. It would have been an advantage had this "Contribution" made its appearance in the trade as a separate publication with a London publisher, at a fixed price, so that all persons interested in the subject in this country might have obtained copies, and recommended the work to their mycological, horticultural, and agricultural friends. M. C. C.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

The Philippine Islands

ENCLOSED you will find:—1. A list of Earthquakes on the Philippine Islands from January to March 1872. As communication is very bad here, and meteorological observations are seldom made and noted down, I may say that most of the earthquakes do not come to our knowledge at all. I believe I do not say too much in expressing my opinion that there may be, at least, one earthquake every day at some one spot in this Archipelago. 2. Description of a Typhoon, which I witnessed at Cebu.

EARTHQUAKES ON THE PHILIPPINE ISLANDS

Since my last communication to the list of earthquakes to your journal (Feb. 5, 1872) I have noted the following:—

1872
Jan. 27, Zambales in Luzon, E.-W., many and strong shocks.
Feb. 7, Camarines on Luzon, twice.
Mar. 5, Manila, weak, 9 A.M. } These two were
6, Province Laguna in Luzon, 9 A.M. } perhaps on the same day, and a mistake has been made in the letter which announced the second.
22, Manila, several very strong shocks.
,, Province Batangas in Luzon.

TYPHOON AT CEBU

April 4, I witnessed a Typhoon in the harbour of Cebu, Philippine Islands, on board H.M.S. *Nassau*, Captain Chimmo. The following gives a short description of it, hoping that the officers of that ship will publish a detailed account of this interesting storm:—

		Barometer	
8^h	A.M.	29·96	Strong N.W. winds, heavy rains.
2^h	P.M.	29·82	
3^h30^m	,,	29·74	
6^h	,,	29·54	Wind, N.W. (4 to 8) rain.
7^h	,,	29·40	Wind and rain ceasing, till
8^h	,,	29·30	some heavy squalls from N.W., followed by perfect calm.
8^h15^m	,,	29·28	Lowest marking of the barometer, till most furious squall from S.E. (11) with heavy rains and lightning. This furious storm lasted about ten minutes, and then, varying from S.E. to S.W. (hardest from S.) diminished slightly; barometer rising rapidly.
10^h	,,	29·76	Wind steadier, squalls less frequent, and heavy rain ceased.
12^h	,,	29·86	Wind steady from S.S.E. (5) dying away till daylight.

About eighteen vessels were thrown on shore, more or less damaged, many houses unroofed, and native huts blown away throughout the island Cebu, and several lives lost. At Horlo in Panay the storm did a great deal of damage too; at Manila it was not observed at all. It is said that there must have been at the same time a storm at Sargoon. Typhoons are very rare as far south as Cebu, and are said not to have been observed for twenty years. This storm proves to be a real typhoon, according to the variation of the wind and the calm betwen it, showing that the centre passed Cuba.

Manila, April 15 ADOLF BERNHARD MEYER

The Conservation of Energy not a Fact, but a Heresy of Science

PERMIT me a few words in reply to Mr. Brooke's strictures in your journal (No. 137) upon my article on "The Heresies of Science" in the *London Quarterly Review* of July last.

Mr. Brooke asserts that in the article "two widely different principles are oddly linked together as heretical dogmas, the doctrine of Evolution and the Conservation of Energy." Now, so far from these doctrines being oddly linked together as heresies, they are not linked at all. It is not the doctrine of Evolution, but the hypothesis of Natural Selection that I affirm to be one of the great heresies of modern science. Evolution is dealt with only so far as is found necessary to prove that the theory of Natural Selection is false. The two heresies named are connected in the article because I found so many physicists employing them to overthrow some of the best established truths in philosophy. Of this Mr. Brooke is perfectly aware, since he expresses regret that "the principle of the Conservation of Energy has by some been misapplied in a fruitless endeavour to supersede the necessity of a creative intelligence."

"The Conservation of Energy a Fact, not a Heresy of Science," is the title of Mr. Brooke's paper. To this assertion I need only oppose some of his own admissions. He complains that the proposition—viz., "that the amount of energy in the world is unchangeable, the sum of the actual or kinetic and potential energies being a constant quantity—has been by some writers rather overstrained." "It may," he adds, "be taken as a postulate, and is probably true, but it is a proposition that is equally incapable of proof or of disproof, because the amount of potential energy in a body can be determined only by its development into actual energy, and cannot therefore be predicated."

Are then our judgments respecting that which Mr. Brooke asserts to be a fact of science only probably true? Surely there is contradiction here. I take it that science is knowledge, and that consequently judgments not accompanied by a conviction of certainty, but merely possessing a higher or lower degree of probability, are altogether outside the sphere of science. As Mr. Brooke accepts the doctrine of the Conservation of Energy as a truth of science, it is not competent for him to maintain that the proposition—viz., "that the amount of energy in the world is unchangeable, the sum of the actual or kinetic and potential energies being a constant quantity"—is equally incapable of proof or disproof, unless he can show that it expresses one of those primary convictions of the mind which constitute the very starting points of human thought. Let Mr. Brooke do this, and there is an end to all discussion on the subject. By one of the laws of thought a proposition which can neither be proved nor disproved, but by other propositions not more evident or more certain, must, by all rational minds, be accepted as true. In this region doubt becomes suicidal by self-contradiction. It is easy to show that the proposition which constitutes the scientific expression of the doctrine of the Conservation of Energy is not the symbol of a primary synthetical judgment. It is really nothing but a truism rendering to the scientific inquirer no higher service than the statement that "every effect must have a cause." In all such cases we grant the truth of the proposition when we grant the definition of either of its related terms. "That everything which begins to be has been produced, immediately or mediately by the power of an intelligent being," is the only philosophic expression of the great law of causation. Stated thus it becomes the appropriate symbol of a primary and necessary synthetical judgment of which every sane mind is conscious. No less a thinker than the late Sir John Herschel held that the doctrine of the Conservation of Energy is a mere truism. It is so as the result of the introduction of what he terms the unfortunate phrase "potential energy."

Mr. Brooke says that "energy is the power of doing work." He does not tell us what he means by work. If he means motion in any of its modes, then he confounds what he holds to

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